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Hydrology and Earth System Sciences Discussions

Interactive comment on "A model for hydraulic redistribution incorporating coupled soil-root moisture transport" *by* G. G. Amenu and P. Kumar

G. G. Amenu and P. Kumar

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Response to Anonymous Referee # 1:

Comment: My major remarks regard the assumptions of the model used by the Authors. Page 3721, line 28. Authors write: This is true both during wet and dry seasons. I think that this is not true because different plant species can have different stomatal regulation during a period of drought. It can happen that during a period of water deficit some species (usually the drought tolerant ones) continue to transpire and photosynthesize also at very low values of water potential and while other species (drought avoiding) stop transpiration by closing stomata. Authors should be more precise and relate this sentence to the species effectively studied in the paper.

Response:

This point is well taken. The paragraph will be revised to read:

"Figure 1 shows a schematic of the hydraulic redistribution. During transpiration from the leaves, the open stomata creates water potential gradient between the leaves and the roots, resulting in net water movement from the soil to the roots and then to the leaves. Water is absorbed from all depths depending upon the potential gradient and passes into the transpiration stream at the leaves. This is true both during wet and dry seasons. When the stomata close, it results in turgor pressure that increases water potential within the plant body. . . ."

Comment: Page 3722, line 8. I can understand the assumptions and simplifications for the model explained at page 3730, line 26 but the concept of hydraulic redistribution (HR) is not clear in the text. If I understood well, HR should allow the water to go from the soil into the roots and vice versa and it is driven by water potential gradients between roots and soil. But the water hardly go out from the roots to the soil because soil water potential is usually much higher than that of root system and there are several barriers for the entering/exit of water in the root system (please see the next point).

Response:

The last statement of the above comments is not clear. The ability of plant roots to discharge water into the soil has been extensively documented and we provide a long reference list in Table 1.

Comment: The Authors write at page 3727, line 9; water goes through complicated pathways in entering the root xylem from the soil media. In fact, water and minerals can normally travel relatively freely through the permeable cell wall matrix of the root cortex (apoplastic route) but this route is blocked by the Casparian strip. In order to reach the xylem, water and minerals must instead take a highly-regulated cytoplasmic (symplastic) route. It is also true that many Angiosperms have a hypodermis with

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another Casparian band (exodermis), which is located in mature regions of the primary roots but only when this latter contains external cells that are in good condition. Moreover, the symplastic route involves special openings between adjacent cell walls called plasmodesmata, and transmembrane protein channels called aquaporins or water channels, that open and close in response to external signals like drought. When the exodermis is present, it represents an important, exterior control point for water and solute uptake. For example, water absorbed by an epidermal cell, and that travels radially inwards towards the xylem by a symplastic way, must before cross the epidermis, the exodermis, various cortical cells, the endodermis and the pericicle, before finally passing from the living cells of the pericicle to the dead ones of the xylem. The Authors should concentrate on the comments of these arguments to give a more complete picture of the soil-plant continuum.

Response:

We thank the reviewer for providing a more complete description of the “complicated pathway” that the water travels from entering the root system to the xylem. However, we disagree that this much detail is required for the modeling goals. As stated on page 3727, line 10 to 23, the entire complexity described above is simplified by assuming an “equivalent membrane” which captures the essential potential difference between the soil and the root system.

Comment: Page 3728, line 6. What about the processes of cavitation and embolism that interrupt water columns and so also the transmission of the tension along them?

Response:

For this modeling effort we have ignored these extreme situations of cavitation and embolism.

Comment: Discussion: do the Authors have any data on the relative contribution of apoplastic and symplastic water pathways and on the existence of apoplastic barriers

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such as the Casparian bands in exo- and endodermis or suberised lamellae in some the species studied?

Response:

No, we don't have any data for such detailed level of modeling.

Response to Anonymous Referee # 2:

Comment: My major concern with respect to this paper is that this practical case uses data from the NARR and IGBP datasets. I have some doubts on the accuracy of these types of data with respect to the model that is used (which is actually a point model). The paper should definitely more elaborate on this aspect. What is the accuracy of these datasets at the point where it is applied? Furthermore, soil moisture data and flux observations from a nearby FLUXNET station is used. Given the fact that both are spatially very variable, is it allowed to use these data as validation in this modelling exercise (it is mentioned in the paper that due to these problems one should not expect a close match)? Furthermore, the validation data only contains near surface soil moisture: is this sufficient to really demonstrate that the hydraulic redistribution, as calculated by the model, indeed represents the actual process? In other words, the section in which the comparison is made with observations should definitely contain more elements on the uncertainty in input, and how this uncertainty translates into uncertainty in the model results, and on the uncertainty in the validation data. Since the validation on real data is not really demonstrating the potential of the model, stating that the model "effectively simulates the soil moisture and water uptake patterns" (see conclusions, line 12, page 2740) is not correct. This statement should be qualified.

Response:

The proposed model can be implemented at the point scale or at a grid scale. In the example presented in the paper we have implemented it at the grid scale and used grid

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averaged properties (see page 3733, line 1). In this form the model can be coupled with mesoscale and climate prediction models. At this scale of implementation the NARR and IGBP data provide good representation, although we agree that if we applied at the scale of a specific vegetation strand, it would not be accurate.

Similar issue arises for validation data. At the FLUXNET tower site, the soil-moisture measurements provide point observations while the latent heat flux incorporates spatial averages to the extent of the “fetch” of the measurement. The spatially averaged latent heat flux better reflect the prediction of the “big root” model (page 3724, line 5) than the point observation of soil-moisture. We agree completely that the soil-moisture comparison with a point measurement do not accurately reflect the validation, they merely ascertain that we capture the general trends present (see last paragraph on page 3739). The latent flux measurements also do not provide the validation data that is needed but the comparison is certainly indicative of the important processes that are captured by the model. In fact, the ability of the model to provide the late summer latent heat prediction that better captures the observed trend provides the confidence in the role of hydraulic redistribution in regulating above ground energy fluxes. In a broad context, capturing this missing process without any calibration reflects our excitement for effectively simulating the soil-moisture and water uptake pattern but perhaps it may be better stated as “the developed model is able to capture water uptake patterns induced by hydraulic redistribution.” We will make this modification in the revised submission.

The issues of uncertainty in input (NARR) data have been discussed in other references [for example see Mesinger et al., 2006]. Assessing the impact of this uncertainty in model performance is the subject of another full length paper. Such an effort will need to assess the uncertainty induced by the input vis-à-vis model parameters, perhaps need comparison with calibrated estimation of parameters to provide the least uncertain estimate for comparison, and perform some sensitivity analyses. We prefer to differ this for another study. Needless to say that like any other model, the performance of this model will vary depending on a variety of factors and these will need to

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be assessed through extensive future research.

Comment: Further, I mainly have some minor remarks. A first, but important one, is that one should not use the same variable name for different variables. I.e. in equation 7, 8 and 9, Krh,ax each time has a different definition (and different units). As this can introduce confusion, I would suggest to, each time, use a different variable name and relate one to another.

Response:

This point is well taken and we'll modify the symbols in the revised submission.

Comment: Equation 10 does not assume that there is no storage (as indicated in line 17 on page 3726)

Response:

The issue of storage change in the root system reflects the ability of the roots to expand to hold more water. We neglect this in the model.

Comment: Explain after equation 15 what variable b means, or, refer to the appendix (equation A2).

Response:

The variable "b" is the exponent in the Brooks-Corey relationship in equation (A1) and (A2). The revised manuscript will reflect this explanation.

Comment: Where does equation 17 come from: reference? If not, explain why this equation has this form.

Response:

In the revised version we will add the following explanation: The equation for throughfall assumes that the amount of rain intercepted by the canopy is a function of the LAI, specifically of the form dq/dL = - constant q, where L is the LAI. The second part of

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equation (17) results from the integration from the top of the canopy (q=q_{rain}) to the ground (q=$q_{through}$). The first part of equation says that the infiltration will not exceed the available capacity of the first soil-layer to take in the water for a computational time step of Δt units.

Comment: Page 3731: add Latin names to the plant species.

Response:

The database we used doesn't provide the Latin names. The names we have used are commonly used in the literature. However, the latin names for Lodgepole Pine and Ponderosa Pine are *Pinus contorta* and *Pinus ponderosa*, respectively.

Comment: Some typing errors: line 28 p. 3721: the stomata close (instead of closes) line 21 p.3734: the stomatal closure (instead of closer) line 26 p. 2736: when the stomata close (instead of closes)

Response:

Thank you. We will fix these in the revised version.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3719, 2007.

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